

THE ROCKEFELLER INSTITUTE
FOR MEDICAL RESEARCH

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NEW YORK 21, N. Y.

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Dear Josh:

The cultures arrived in good shape and all of the lyophil tubes contained viable bacteria. Also received your two most interesting letters. ~~which~~ The pressures of the moment prevented the prompt reply which they deserved.

Let me try to bring some light on the S. para B story. The Boulgakov phages arrived when I was in Boston. At that time, to test them, you chose some cultures including ~~the~~ an S. paraB. I presumed that you chose the Edwards 3 but it is possible that you did not. I had in the rack in the refrigerator some para B's from your old Yale collection and perhaps it was one of these. The culture was given an SW number(533) and was lyophilized with the others. It was filed with the serotypes as I thought it was a duplicate. The unmarked S. para B should be it, though why it isn't labelled 533 is beyond me. I too was suspect of the parentage of 534 as when tested it reverted to a stable 1,2 unlike its supposed parent, but never got around to asking you about it. I shall send you all of the para B's I have. Test of the antigenic character of the unmarked culture should help somewhat.

I am very excited by the kinds of analyses you are doing with the flagellar system. I had deplored the fact that transduction couldn't be used for other than the most elementary ~~of~~ genetic analysis; allelic substitution. It would seem however that there are many of the most interesting gene interactions at this level.

Once alternate flagellar states can be identified with non-allelic genes and the suppressed state is not released in transduction, simple mutation really fails as an explanation of phase variation. I must admit that the significance of ~~the~~ latter point almost completely escaped me until the arrival of your last letter. All I can recall is a slight uneasiness ~~about the matter~~ as to why typhimurium II lacked i components. With regard to monophasicity; isn't it possible that monophasic strains completely the alternate genetic site. For example; tymurII not only doesn't transduce i to 543 but also not 1,2, although 534 does. Thus it would seem that the 1,2 are not allelic, the tymurII at the non-specific locus (lacking an homologous site) while the 534 is at the specific site.

I too deplore the terms auto- and allogenic transformations. Perfect example of the useless coining of neologisms. Speaking of transformation, Hotchkiss is now obtaining 5% ~~xxx~~ and is able to demonstrate saturation effects. He will soon test the independence of different factors in "multiple" transformation.

I have been trying to be unbiased with regard to the Plough story especially when questioned in seminars however I agree most heartily with your succinct statement about it and shall ask Bernie to try to test the strains. I don't think it would be in good taste for me to do so directly.

I now have a good antiPLT-22/2 serum and shall send you some with the para B's. It has a K of 80 for the phage and the tests for FA are still in progress.

The interference experiments are leading me on a merry chase. On repetition with no interference with extraneous phage all of the twenty-five tested transducees had the parental phage. The ratio of phage to FA was 10^6 to 1 at dilution, ~~however~~ and the assay was linear up to a multiplicity of one to one at which point the recovery of FA falls off rapidly. There is no constant ~~x~~ relationship between the recovery of F^{+} and the multiplicity but the trend is the higher the multiplicity the smaller the percent of the transducees recovered. The mutant phage adsorbs as well as the parent and reaches the same

X (see over)

multiplicity at saturation. This might be explained as follows; in order for a transduced cell to survive it must throw out the mutant, as the multiplicity increases the probability of this goes down. However as with the U.V. activation I can not explicitly demonstrate bacterial death after multiple phage infection. Regardless of this latter point U.V. inactivation might straighten out the curve. The separation of phage and FA is very clear cut.

Point of information: Boyd's 1404 is an excellent indicator for PLT-22 especially with regard to plaque morphology.

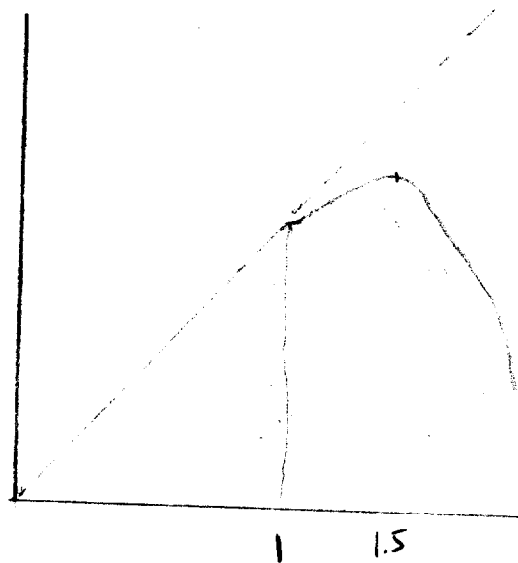
Marked virulent and avirulent cells are now available and the in vivo and in vitro tests are in progress. The streptomycin resistant mutants show a five decade difference in virulence although there are no nutritional or antigenic differences between them and the virulent parent. In this sense I hope we may be able to transduce virulence.

Best regards

Sincerely;

Norton

Parent phage



*Probably linear but
I hate to prove it by
my points*

8-10